Meta-analysis of livelihood impacts of payments for environmental services programmes in developing countries

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Abstract

Payments for environmental services (PES) programmes have been widely promoted over the last few decades in many developing countries. Improving the livelihoods of environmental services (ES) suppliers is not only seen as a side benefit but is often considered a prerequisite for the viability of PES. Yet, the ability to draw 'overview lessons' over the impacts of PES on livelihoods from literature review studies remains limited. To overcome these shortcomings, we undertake a meta-analysis of causal statistical studies on the effects of PES on the livelihoods of ES suppliers in the developing world. The set-up of our meta-analysis allows us to draw more conservative but more reliable and generalisable overview lessons. Our findings suggest that PES programmes are likely to have positive but modest livelihood impacts on ES suppliers. Further, several institutional characteristics of PES are found to be correlated with more favourable livelihood impacts, such as high payments, high degree of voluntary participation, low transaction costs and better access to alternative income sources. Lastly, our results highlight the importance of controlling for unobservable confounders when undertaking original evaluation studies on the impacts of PES. These factors should be incorporated in the design, implementation and evaluation of PES.

Keywords: Payments for environmental services programmes, ecosystem services, livelihood impact, meta-analysis, policy evaluation methods, conservation.

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1 Introduction

The last decade has witnessed a rapid growth of payments for environmental services (PES) programmes in developing countries (Ferraro, Lawlor, Mullan, & Pattanayak, 2012; Pattanayak, Wunder, & Ferraro, 2010). Typically PES schemes entail a form of Coasean transaction in which environmental service (ES) users make payments to ES providers, in exchange for the provision of ES or pre-agreed actions intended to deliver ES (Engel 2016; Wunder, 2015).[1] In principle a crucial pre-condition for the viability of PES schemes is that the benefits accruing to the suppliers of ES should exceed the costs of ES provision (Engel, Pagiola, & Wunder, 2008; Pearce, 2004). In other words, the net livelihood impact on payment recipients should be positive. This pre-condition is particularly crucial when PES schemes are applied in the developing world, as in this context these policies often have the dual function of addressing environmental goals and alleviating poverty (Wunder, Engel, & Pagiola, 2008). It is, thus, important to assess and understand the magnitude, direction (positive or negative) and determinants of livelihood impact of PES polices in the developing world.

Yet, although there is a substantial body of empirical work that aims to assess the impact of various PES schemes, our ability to draw robust 'big-picture' or 'generalisable' lessons remains limited. Literature reviews or synthesis papers such as those by Grieg-Gran (2005), Jack (2008), Wunder (2008), Pattanayak (2010), Miteva (2012), Tacconi (2013), Alix-Garcia (2014), Engel (2016), Hejnowicz (2014), Samii (2014), Börner (2017), Ma (2017) and their co-authors are able to provide a *qualitative* assessment of the environmental and socioeconomic impacts of PES. Though their inferences over actual PES impact are informative, they remain very case specific. We are unaware of any robust *quantitative* syntheses of available empirical evidence (such as via meta-analysis) of the livelihood impact of PES. What has perhaps hindered this research agenda of deriving more overview lessons is that the possible universe of 'empirical evidence' is vast and quite heterogeneous. Empirical assessments on the impacts of PES studies use different forms of data and analytical methods at different levels of scientific rigour. Further, a large proportion of the empirical work on evaluating the outcomes of PES has the limitation of not being able to credibly trace the causal relationship between a PES and its observed 'effects', either on the affected communities or the environment (Blackman, 2013; Ferraro et al., 2012; Miteva, Pattanayak, & Ferraro, 2012). That is many empirical studies provide purely correlational findings and do

[1] This definition confines PES to 'payments', so that they can be distinguished from institutional reforms (such as bestowing upon individuals or communities the right to benefit from natural resources) and other incentive-based environmental policy instruments (such as environmental taxes) that are geared towards ES supply. It also conceptually separates PES from ecotourism and eco-certification, which were once labelled as 'indirect PES' (Ferraro & Kiss, 2002). In such situations, the delivery of ES is largely an ancillary benefit, which is not being directly traded. This definition also does not include the more general understanding of PES as any policy that provides any incentive for the provision of ES (e.g. see Muradian et al 2010, Sommerville et al 2009)

not adequately control for confounding factors that could lead to observed impacts and which are unrelated to the PES itself (Ferraro and Pattanayak, 2006, Baylis et al 2016). Combining empirical results from studies that use such varied methods and data into a meta-analysis would lead to results which would be unreliable and of limited informational value.

This paper addresses these challenges in deriving reliable overview lessons from the existing empirical body of work on PES by making two key contributions. First we perform perhaps the first meta-analysis of empirical studies that aims to explore the impact of PES programmes on the livelihoods of ES suppliers in developing countries. Our analysis adds to more qualitative past attempts that have relied mostly on literature reviews. Secondly, we include in our analysis only the evidence from *ex post* impact evaluation studies that use statistical methods that explicitly aim to assign a causal link between PES and observed outcomes. The quality of any meta-analysis is only as good as its inputs (Glass, 1976) so keeping with best practise guidelines we confine our analysis to include studies that entail a minimal level of methodological similarity.

In particular, we use studies that employ impact or programme evaluation methods that seek to identify the (treatment) effects of PES on livelihoods whilst controlling for and isolating from other confounding factors. These methods include experimental methods which control for confounders through randomly assigned treated and control groups, and quasi-experimental approaches such as matching, difference-in-differences, instrumental variable and regression discontinuity methods (Ferraro & Hanauer, 2014; Greenstone & Gayer, 2009; Imbens & Wooldridge, 2009; Khandker, Koolwal, & Samad, 2010). The need for focusing more on such methods in assessing conservation policies (and PES in particular) has been widely and vigorously argued for in recent literature (e.g. Ferraro & Pattanayak, 2006; Greenstone & Gayer, 2009; Pattanayak et al., 2010; Miteva et al., 2012; Cowling 2014; Baylis et al., 2016; Börner et al., 2017; Sills et al., 2017). Admittedly, impact evaluation methods are not without flaws nor are they the only scientifically credible approaches for assessing policies. Yet, for the purposes of undertaking a valid meta-analysis it is imperative to include studies that share a minimal common methodological ground. This reduces both the subjectivity in selecting the included studies but also the 'noise' in the raw data to be used. This rationale conforms with the 'best evidence synthesis' approach, as advocated by the seminal publications on meta-analysis by Hunter, Schmidt and Jackson (1982) and Slavin (1986) but also by best practise guidelines within economics (Stanley and Doucouliagos 2012; Nelson & Kennedy, 2009). In the present meta-analysis, we focus on empirical studies that are explicitly geared towards addressing the issue of attribution via statistical methods. In doing so, we are better positioned to draw more reliable inferences about the causal relationships between PES programmes and key outcome variables. Our approach aims to maximise the possibility of producing clear and credible (albeit more conservative) overview lessons from the existing body of empirical work.

The quantitative nature of our study and the focus on causal statistical evidence distinguish it from other qualitative syntheses and literature reviews. Although the studies of Oltmer (2000), Brouwer (2011), Ezzine-de-Blas (2016) and their co-authors provide rare examples of quantitative syntheses assessing PES programmes, they do not focus on livelihood impacts nor do they limit their analysis to causal statistical studies. As such, the present study is able to draw more conservative but perhaps more reliable and more generalisable overview lessons on the likely impact of PES on livelihoods. Our analysis provides evidence on how to design and implement such programmes so as to enhance the livelihood impact and the long term viability of PES programmes. Further, our results shed light on the influences of different (causal) evaluation methods on the measurement of livelihood impact which provide lessons to those working on policy assessment of PES.

This paper proceeds as follows. Section 2 sets out our main hypotheses based on stylised facts on the livelihood impact of PES programmes as derived from past empirical studies and literature reviews. Section 3 describes our data used in the meta-analysis and Section 4 discusses our empirical strategy. The results are presented in Section 5, and the paper concludes in Section 6.

2 Assessing livelihood impact of PES programmes

Based on the aforementioned literature and qualitative reviews of past experiences with PES, we discern four main hypotheses with respect to livelihood impacts of PES on ES suppliers. These hypotheses stipulate *a priori* positive impacts of PES on livelihoods. Moreover, we identify additional PES attributes that could impact livelihoods but for which the direction and magnitude are more ambiguous *a priori*. Lastly, we explore in the meta-analysis the significance of specific features of the primary impact evaluation studies.

2.1 Positive PES impacts on livelihoods

PES programmes are usually assumed to benefit ES suppliers because the received definition of PES suggests that – provided the scheme is voluntary – the agreed payments should be higher than the opportunity costs of ES suppliers and lower than the willingness to pay (WTP) of ES users (Engel et al., 2008, Engel, 2014). In practice, it appears that the payments tend to be closer to the opportunity costs of ES suppliers, or the minimum willingness to accept (WTA) (Engel 2016, Pagiola, Arcenas, & Platais, 2005), and the gains from supplying ES, if any, may likely be insignificant. The possible reasons for this are twofold. First, it is believed that ES users tend to be in a better negotiating position because they are often relatively fewer in number and better-informed (Wunder, 2008). Secondly, the opportunity costs of ES suppliers are relatively easier to estimate, at least on average, compared with the WTP of ES users (Pagiola et al., 2005). Moreover, the benefits of ES suppliers may be diluted or even overturned by general equilibrium effects. For instance, a PES programme intended to deliver ES through agricultural land retirement may reduce local food supply and raise food prices, which would adversely affect the well-being of ES suppliers (Zilberman, Lipper, & Mccarthy, 2008). Yet, even if we observe non trivial livelihoods improvements of ES suppliers in developing countries, it remains an open empirical question whether these reflect other confounding influences or if they can be attributed to the PES payments *per se*. Since our meta-analysis focuses on studies that have tried to address the issue of attribution, we will attempt to provide a more reliable assessment of this fundamental question, namely:

Hypothesis 1: PES programmes have positive livelihood impacts on ES suppliers.

In most cases, direct payments themselves provide the main mechanism by which PES programmes are to influence the livelihoods of ES suppliers. It is, thus, to be expected that higher payments (either in cash or in kind) would make ES suppliers better off (Pagiola et al., 2005). However, such additional benefits could be compromised if higher payment rates decrease the demand for ES (Ferraro, 2008). Further, there is considerable theoretical work suggesting that transfers schemes to farmers (such as agri-environment and agro-forestry payments) have general equilibrium effects (affecting prices of factors and commodities) in situations where economies are imperfectly integrated in regional economies (e.g. Angelsen et al. 2001). In such setting the expected correlation between higher payment rates and more favourable livelihood impact needs to be empirically substantiated. We, thus, explore through our meta-analysis whether the existing available empirical evidence from impact evaluation studies support the following hypothesis:

Hypothesis 2: PES programmes have positive livelihood impacts on ES suppliers if the programmes have higher payments.

Further, it would be reasonable to expect that as long as rational ES suppliers voluntarily join PES programmes, they cannot be worse-off (Grieg-Gran et al., 2005; Pagiola et al., 2005; Wunder, 2008). In other words, voluntary participation can be regarded as a proxy indicating that benefits outweigh opportunity costs for ES suppliers. Yet, the development economics literature suggests that households (due to institutional failure reasons) often voluntarily choose sub-optimal equilibria with respect to land use, labour supply, consumption, savings and investment behaviour (e.g. Banerjee and Newman, 1994; De Janvry and Sadoulet, 2006). Sometimes ES suppliers who opted to participate at their own will were in effect making a sub-optimal decision that adversely affects their livelihoods. This is likely because they did not fully account for the costs and benefits of participation due to, (1) being given false and/or incomplete information about the terms of the programme, (2) lack of information over their true opportunity costs of enrolment, and (3) unforeseen changes in the costs and benefits over time (Wunder, 2008). Hence, the voluntary nature of a PES scheme does not necessarily suffice to infer positive livelihood impact. Our meta-analysis will thus aim to infer if empirical evidence lends support to the hypothesis:

Hypothesis 3: PES programmes have positive livelihood impacts on ES suppliers if their participation is made on a more voluntary basis.

Moreover, transaction costs have long been deemed a major threat to the viability and efficacy of PES (Pearce, 2004). The Coase Theorem suggests that sufficiently low transaction costs constitute a precondition for PES to function at all. In light of that, the presence of substantial transaction costs would deplete the payments available to ES suppliers and hence lower their gains. For example, in the N'hambita PES programme for carbon sequestration (Mozambique), about one third of the carbon revenues are exhausted by local transaction costs, and another third are paid to international brokers and commission agents who help to sell the carbon offsets (Jindal, 2012). Similarly, a Cambodian PES programme for bird nest protection spends 20-30% of its budget on monitoring (Clements, et al., 2013). Worse still, groups that are more likely to benefit from ES provision (due to lower opportunity costs) are often subject to higher transaction costs, as they are typically small landholders living in remote regions (Pagiola et al, 2005; Wunder, 2008). These theoretical insights and practical experiences are suggestive of limited livelihood benefits to ES suppliers in situations where transaction costs associated with the PES are nontrivial. Our study attempts to provide an overview assessment of what current best available empirical evidence has to say about the hypothesis:

Hypothesis 4: PES programmes have positive livelihood impacts on ES suppliers in the absence of significant transaction costs.[2]

2.2 Other PES attributes affecting livelihoods

Beyond these hypotheses on the positive impacts of PES on livelihoods, the literature suggests additional PES attributes that could also have implications for the livelihoods of ES suppliers. However, there is more ambiguity as to the direction of the influences of these factors.

To start with, there could be either positive or negative influences on livelihoods if a PES programme is financed and executed by the government, compared with non-governmental bodies and individuals. On the upside, government-financed programmes are more likely to have built-in additional livelihood improvement objectives (Wunder et al., 2008). That said, governments often have more bargaining power in the negotiation process. Budget considerations may tempt them to set the payment rate below the equilibrium price of the ES market, or even lower than the opportunity costs of ES suppliers (in cases where we don't have entirely voluntary participation).

Moreover, beyond direct payments, many PES programmes provide additional income sources to ES suppliers which were previously unavailable. For example, farmers receiving direct payments for afforestation may also be allowed (as part of the PES agreement) to sell specific types and quantities of non-timber forest products extracted from the conserved areas. Alternative income sources could not only influence the livelihoods of ES suppliers during the programme period, but also affect the sustainability of ES provision and livelihood improvement after the payments expire (Grosjean & Kontoleon, 2009). On the other hand, in some PES programmes, alternative income sources are coupled with less attractive payment provisions. For instance, during the first stage of the Sloping Land Conversion Programme (SLCP) in China, ES suppliers were paid less if they agreed to convert enrolled cropland plots to 'economic forests', compared with those who were not granted such rights (Xu, Yin, Li, & Liu, 2006). Economic forest entailed planting fruit trees, in which case people enrolled in the SLCP were allowed to harvest forest products as an alternative income source. Our

[2] When testing this hypothesis, we consider a PES programme to be subject to 'significant transaction costs' if over 10% of the total budget is explicitly spent on administrative affairs such as coordinating ES users and suppliers, arranging contracts, making payments and monitoring.

analysis explores the overall livelihood impacts of PES programme if the contract provided to the ES suppliers the possibility of exploiting alternative income sources.

Furthermore, previous literature has discussed the implications on livelihoods if the PES payments are partly or mostly invested in communal infrastructure development. Such infrastructure development may tend to benefit ES suppliers indirectly and in the long term, but is likely to be delivered at the cost of deductions of immediate cash (and in-kind) payments. Our meta-analysis explores whether current empirical studies can shed light on the net effect on livelihoods when such non-income related benefits are built into the PES contract.

Lastly, if a longer time period has passed since the commencement of a PES programme, it is possible that an impact evaluation study may find more favourable livelihood impacts. More payments could be transferred to ES suppliers during a longer implementation period, which are likely to cover the initial investments required for ES provision and accumulate into the net benefits of ES suppliers. Moreover, certain extra benefits would only occur when the programme has been running for several years. For instance, the reforestation contracts in the Costa Rican national PES programme allow the participants to log trees after an initial time period (Pagiola, 2008). Another example is the benefits from favourable institutional changes triggered by some PES programmes such as relaxing the constraints on land tenure security and liquidity (Arriagada, Sills, Ferraro, & Pattanayak, 2015; Uchida, Rozelle, & Xu, 2009; Wunder, 2008). On the other hand, it is also possible that the benefits of ES suppliers would fade out in the course of time if the payments are devalued by inflation or if the contracts terminate, in which case the former ES suppliers might revert back to their old (often sub-optimal) production decisions. Moreover, in some other cases, PES-like programmes were suspected to have adverse effects on institutions in the long run, such as incentivising recentralisation of forest rights (Phelps, Webb, & Agrawal, 2010) or tightening liquidity constraints (Jayachandran, 2013).

2.3 Features of primary impact evaluation studies

Aside from these features of PES *per se*, estimates of their livelihood outcomes could also be influenced by features of the primary empirical impact evaluation studies themselves. We now turn to discuss these features.

First, estimates of the livelihood impact from field and quasi-experimental studies could be biased if they are measured without controlling for observable and unobservable confounders, or if they were derived from studies that do not use baseline data. It is possible that the treated and control groups used in the study are not randomly assigned, and there are some systematically different characteristics of the two groups that could influence the treatment outcomes. Hence, if these confounders are not controlled for, the estimates of the outcomes could be biased (Ferraro & Pattanayak, 2006; Imbens & Wooldridge, 2009). Similarly, some of the baseline characteristics of the treated and control groups before the treatment could also influence the treatment outcomes. These initial characteristics can be controlled for if baseline data is available. We, thus, control in our meta-regressions for whether observable/unobservable confounders are included, and whether baseline data are gathered and used in the primary data studies.

Moreover, our meta-analysis examines whether the estimates of the livelihood impact is influenced by whether non-income indicators (such as consumption expenditure, value of durables and assets) are used in the primary impact evaluation studies. Ample literature suggests that it should be useful to include non-income indicators to measure welfare impacts, since income is difficult to be measured accurately in less-developed regions due to complex production patterns and self-employment, while also the linkage between income and welfare is not always clear (Haughton & Khandker, 2009).

Last but not least, the publication status of the primary evaluation studies could be correlated with the estimates of livelihood impacts (Card, 2012). For instance, it is possible that positive evaluation results are more likely to be published. Our study has sampled both published and unpublished evaluation studies to control for potential publication bias.

3 Data Description

3.1 Selection of primary impact evaluation studies

As noted in the introduction, the studies we include in our analysis have all utilised sound econometric methods that explicitly aim to address (to varying degrees) the issue of attribution (i.e. how can we directly trace livelihood impacts back to PES incentives). These studies can be distinguished from correlational ones as well as others that use more descriptive and qualitative methods (e.g. structured interviews etc.). Delineating the universe of possible studies is amongst the most crucial initial steps of any meta-analysis. The balance that meta-analysts should reach is between statistical power on the one hand and introducing noise or measurement error in the dependent variable on the other. One could augment the possible pool of studies and then include a dummy variable on whether the study adopts a sound causal statistical method or not. Yet, the dependent variable that we are interested in (namely impact on livelihoods) would be misspecified if it came from studies that do not share the minimum methodological common ground of aiming for causal inference. A similar logic is followed in meta-analyses conducted in the medical sciences where causal studies are not normally included with correlational ones. By focusing on studies that adopt causal statistical methods we enhance the likelihood of achieving a ‘best evidence synthesis’. This will facilitate a clearer (albeit more conservative) judgment on which assertions found in the PES literature can and which cannot be supported by the current body of best available empirical evidence. Based on this reasoning, to be included in our meta-analysis, a study would have to have 1) conducted an *ex post* evaluation of the livelihood impact of PES programmes on ES suppliers at the household level, 2) employed causal statistical methods as described above, and 3) measured the impact on at least one indicator which reflects the overall livelihood status such as the total income, total consumption expenditure and/or gross value of household assets.

We compiled studies for the meta-analysis from several different literature sources, including academic digital databases and online libraries of relevant governmental and non-governmental organisations. We searched for both published and unpublished studies, and assessed the inclusion eligibility of both. Only studies written in English were explored. Details of the literature sources and searching strategies are listed in Appendix A. Our literature search and selection process provided 161 observations from 27 studies on 15 PES programmes (see Appendix A). These figures are in line with best practise guidelines and published meta-analyses in economics (Stanley and Doucouliagos 2012; Card 2012). The vast majority of our studies (23) are based on quasi-experimental methods. There are 19 of these studies published in peer-reviewed journals or books. Many of these studies or their accompanying research outputs are published in leading academic journals such as *Science*, *Proceedings of the National Academy of Sciences*, *American Economic Journal: Applied Economics*, and *American Economic Journal: Economic Policy*. Nearly all these studies are published or written after 2010 (with only three exceptions). This tendency in the data confirms that the interest in causal statistical evaluations of PES is part of a relatively more recent but growing research agenda. In fact, this recent rise in the number of empirical impact evaluation studies on PES from the developing world is what enabled us to undertake a reliable meta-analysis in the first place.

The sampled studies involve major PES programmes in developing countries in Asia, Africa and the Americas (as in Figure 1). The geographic focus is dominated by Asia (15 studies), and there is an absence of evidence from Oceania. Review studies on other conservation programmes have found similar geographic distributions of study sites, such as the systematic review on community forest management programmes by Bowler et al. (2010).

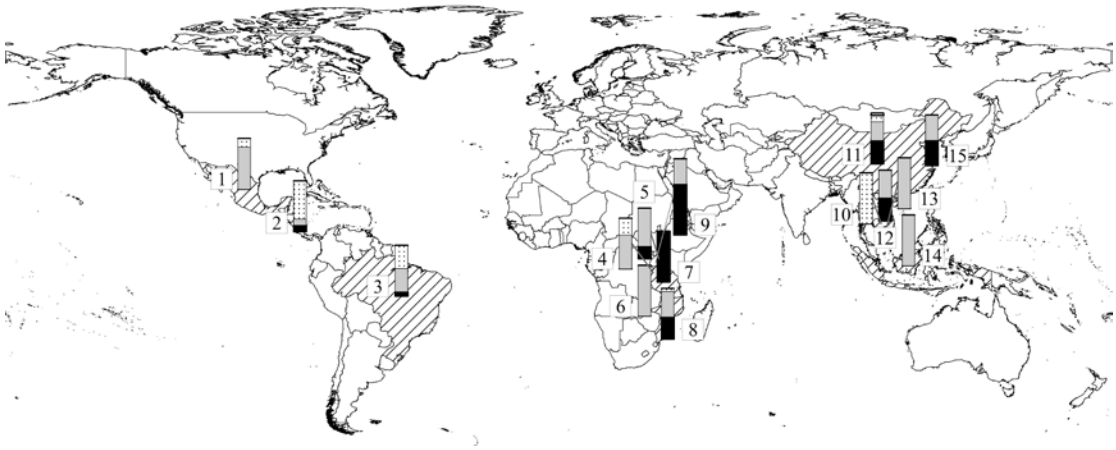

Figure 1 Geographic locations of the PES programmes in the meta-analysis

Note:

1. Each bar uses greyscale colours to represent the proportions of different livelihood impacts reported for each PES programme (white segment: significantly negative; dotted segment: insignificantly negative; grey segment: insignificantly positive; and black segment: significantly positive).

2. PES programmes: (1) Mexican Payments for Hydrological Environmental Services (PSAH); (2) Costa Rican Payments for Environmental Services (PSA); (3) Brazilian Forest Conservation Grant Fund (BFP); (4) Rwandan Randomised Controlled Trial (RCT); (5) Ugandan Trees for Global Benefits (TGB); (6) Malawian RCT; (7) Tanzanian Equitable Payments for Watershed Services (EPWS); (8) N'hambita (Mozambique) PES; (9) Ugandan RCT; (10) Cambodian Bird Nest Protection Programme (BNPP); (11) Chinese Sloping Land Conversion Programme (SLCP); (12) Vietnamese Payments for Forest Environmental Services (PFES); (13) Vietnamese RCT; (14) Lombok (Indonesia) PES; (15) Chinese Paddy Land-to-Dry Land (PLDL).

As it is often the case with meta-analyses in the social sciences, our data included cases where multiple observations were derived from the same study. A survey of 140 meta-analysis studies reveals that on average 6.9 observations are used per primary study (Nelson & Kennedy, 2009). Hence, our analysis (that uses 6 observations per primary study on average) is in accord with current common practice.

3.2 Data Extraction

The livelihood impacts of the PES programmes measured by the primary studies (or the 'effect sizes'), were coded to form a categorical dependent variable (y), reflecting significant negative impact (y = 0), insignificant negative impact (y = 1), insignificant positive impact (y = 2) and significant positive impact (y = 3). The wording 'significant' suggests that an estimate of the livelihood impact (no matter positive or negative) is statistically different from zero, and 'insignificant' otherwise. Such coding of the dependent variable was necessary given that different livelihood indicators were used across the primary studies or because similar indicators were not measured in the same way. The potential influencing factors of the effect sizes, or the 'moderators', include the characteristics of PES programmes and evaluation methods discussed above. The original studies and supplementary sources (as described below) provided straightforward and unambiguous information for us to calculate or code these moderators without requiring any arbitrary interpretations. Appendix B summarises the coding methods and descriptive statistics of these moderators.

The vast majority of the required information was derived from the 27 impact evaluation studies. Upon the occurrence of missing values, we would firstly contact the authors of these papers for such information. In case the authors were unreachable, we would impute the missing information according to alternative sources. Further details are documented in Appendix B.

4 Estimation Methods

The first hypothesis examines the central objective of most meta-analyses – to explore the main 'effect sizes' of interest (Card, 2012) – and which in this study refers to the typical reported livelihood impact of PES on ES suppliers in developing countries. The typical 'effect size' is firstly assessed by the mode and mean values of the 161 estimates of effect sizes taken from the sampled primary studies. However, as discussed before, we derived multiple observations from one single primary study or PES case. Placing equal weight on each of the 161 data points may likely bias our results towards those studies or PES cases reporting many results. Following best practice guidelines (Nelson & Kennedy, 2009) we checked the robustness of our results by using the mode (or mean) of each study (rather than all its estimates) to explore the typical 'effect size'. We then repeated this procedure at the PES programme level.

In order to test Hypotheses 2–4, a series of meta-regression models were estimated to examine the relationship between the effect sizes and the variables of interest (the moderators summarised in Appendix B). The dependent variable is coded as ordinal categorical variables taking on the four ordinal levels of effect sizes. Model 1 in Table 2 was estimated as an ordinal logit model using unweighted data. As noted above, this model is likely to overweight those studies or PES programmes that contribute more data to our meta-analysis. We accounted for this issue by weighting our data using the inverse of the number of observations derived from the same primary study or PES programme (Nelson & Kennedy, 2009), so that each study or each PES programme would carry equal weight. Models 2 and 3 in Table 2 were estimated on the weighted data. A positive coefficient in these ordinal models suggests that increasing the corresponding variable would – *ceteris paribus* – enhance the probabilities of observing more favourable livelihood outcomes. More formally, an explanatory variable's coefficient captures its effect on the likelihood of a 'higher rated livelihood outcome', which is measured in log odds: $ln\frac{Pr(Higher\ rated\ livelihood\ outcome)}{1-Pr(Higher\ rated\ livelihood\ outcome)}$. In our dataset, a 'higher rated livelihood outcome' can refer to any one of the three situations: (1) a significant positive impact (y = 3), (2) a positive impact (y = 3 or 2), or (3) a positive impact or an insignificant negative impact (y = 3, 2 or 1). The statistical validity of the ordinal logit model requires the parallel regression assumption that in all three situations a moderator's effect on the log odds of this 'higher rated livelihood outcome' is the same (Greene &

Hensher, 2010; Long & Freese, 2014). As this restrictive assumption complicates the interpretation of the regression results, we sought to relax it and facilitate better understanding of the meta-regression findings by formally recoding the effect sizes into a binary dependent variable where by y = 1 for significant positive livelihood impacts, and y = 0 otherwise. Models 4–6 in Table 2 are the binary counterparts models of the ordinal ones shown in Models 1–3. A coefficient in these binary logit models indicates a moderator's influence on the probabilities of significant livelihood improvements for ES suppliers. Such results are more easily interpretable than those of the ordinal models. All estimation was undertaken in Stata.[3]

5 Results

We begin with testing the first hypothesis on the typical livelihood impact of PES. The bar charts in Figure 1 visualise the proportions of different livelihood impacts reported for each PES programme. A visual examination would reveal that positive but statistically insignificant livelihood impacts (represented by grey segments) are typical or dominating. This observation is confirmed by formal statistical evidence displayed in Table 1. The first column of results in Panel 1 shows that if we allow for multiple observations drawn from each primary study (or PES programme) and treat them equally, insignificant positive livelihood impacts (y = 2) are most frequently observed, followed by significant positive impacts (y = 3). This tendency becomes even more evident in the next two columns of results, which are derived from study-level (or PES-level) mode values so as to eliminate the disproportionate weights carried by studies (or PES programmes) that report a large number of results. Following the same structure, Panel 2 contains the mean effect sizes respectively at the estimate, study and PES levels. We find that we cannot reject the hypothesis that average $y = 2$ but equally we can reject that y = 0 or 1 or 3. These results indicate that taken together the empirical evidence suggests that PES programmes tend to achieve positive but mostly insignificant livelihoods improvements. This finding remains stable upon dropping observations on involuntary ES suppliers (as in Table C1 in Appendix C). These results taken together provide some albeit weak evidence in favour of Hypothesis 1. This finding is consistent with the logic of PES that the payment should at least cover the cost of ES provision, or in other words, voluntary participants should not be worse off.

[3] In order to test the robustness of our findings to alternative definitions of the binary dependent variable, we constructed this variable in a different way, namely y = 1 for positive livelihood impacts, and 0 otherwise, and estimated Models C1–C3. Further, PES programmes in theory should entail full voluntary participation of ES suppliers (Engel, 2016; Wunder, 2015). Yet, in reality the degree of voluntariness varies and often cannot be fully observed. For example, in China's SLCP the degree and extent of voluntary participation remains an indiscernible and contentious issue (Groom & Palmer, 2012; Wunder et al., 2008). Completely excluding these studies would entail a significant informational cost, as their samples do at least partly consist of voluntary participants. Despite that, we formally assessed the influence of using such 'mixed' data by repeating all our analyses after dropping these studies. These results are reported in Appendix C.

Table 1 Livelihood impacts of PES programmes

Livelihood impact	Unit of analysis: Estimate	Study	PES
Panel 1: Mode			
Number (percentage) of significant negative estimates: $y = 0$	3 (2%)	0 (0%)	0 (0%)
Number (percentage) of insignificant negative estimates: $y = 1$	32 (20%)	4 (13%)	3 (18%)
Number (percentage) of insignificant positive estimates: y = 2	***77 (48%)***	***15 (50%)***	***10 (59%)***
Number (percentage) of significant positive estimates: $y = 3$	49 (30%)	11 (37%)	4 (23%)
Total number (sample size)	161 (100%)	30 (100%)	17 (100%)
Panel 2: Mean			
Mean [standard deviation]	2.07 [0.76]	2.13 [0.57]	2.07 [0.52]
Sample size	161	27	15
p-value from *t*-test: mean = 0 (significant negative impact)	0.00	0.00	0.00
p-value from *t*-test: mean = 1 (insignificant negative impact)	0.00	0.00	0.00
p-value from t-test: mean = 2 (insignificant positive impact)	***0.26***	***0.25***	***0.63***
p-value from *t*-test: mean = 3 (significant positive impact)	0.00	0.00	0.00

Note: In Panel 1, for the study level analysis (the second column of results), the total number of studies (30) exceeds 27 because the evaluation results of a few studies have multiple mode values. The same holds for the PES level analysis (the third column of results).

We next turn to the meta-regression coefficient results presented in Table 2. We examine if coefficient estimates for the same explanatory variables across different models remain stable (i.e. retain the same sign and roughly similar significance levels). The positive coefficient of 'payment' provides corroborating evidence for Hypothesis 2 that PES programmes have more favourable livelihood impacts on ES suppliers if the programmes have higher cash and in-kind payments. This result is statistically significant in most of the models in Table 2, except in Models 3 and 6 where we assign less weight to data obtained from the same PES programme but from different studies (using the inverse of the total number of observations on the same PES programme). In fact, a PES programme may offer different types of contracts that entail different payment rates and/or durations, such as the N'hambita PES (Mozambique), the PSA (Costa Rica) and the SLCP (China). Many studies in our dataset intentionally assessed and compared the outcomes of different contract types (e.g. Jindal et al., 2012; Liu and Henningsen, 2016; Morse, 2007). These data, despite being derived from the same PES, bring in additional information about how livelihood impacts change in response to different payment rates. Weighting them less in the meta-regressions has likely weakened our statistical power to detect the nexus between payment levels and livelihood impacts. We hence take the results from the models estimated with this particular weighting of the data with more caution.

Recall that a coefficient's magnitude translates into changes in the log odds of a more favourable livelihood effect, which applies to both ordinal and binary logit models. Therefore, the coefficient of 'payment' in Model 1 indicates that if the variable is increased by one unit, the log odds of a higher rated livelihood outcome would be increased by 1.04.[4] We can interpret our binary models in the same manner, except that in these models the 'higher rated livelihood outcome' specifically refers to significant livelihood benefits.

Hypothesis 3 stipulates that ES suppliers would be better off in case of higher degree of voluntary participation. Our meta-regression results lend strong support to this hypothesis, as reflected by the positive and statistically significant coefficient of the variable 'voluntary participation' throughout all models. The magnitude of such effect further justifies this hypothesis, which is greater than that of 'payment' in every model. We formally explored the relative effects of the two moderators using the predicted probabilities of different livelihood impacts given by Model 1 (in which the coefficients of the two moderators are closest to each other). Several interesting findings emerge. For example, it is found that a fully voluntary programme would be more likely (y = 3 with probability = 53%) to deliver significant livelihood benefits compared to a semi-voluntary programme that doubles the payment premium (y = 3 with probability = 41%).[5] Further, the highest average predicted probability for the dependent variable, y, acquiring one of the four assigned values (0, 1, 2, 3), is found when y = 2 (with probability of 51%). This is followed by the probability of y = 3 with probability of 43%.[6]

We further noted above that the typical livelihood impacts tend to be positive but insignificant even after dropping all PES cases where participation is suspected to be – partly at least – involuntary. Lastly, as previously mentioned, it has been suspected that voluntary participation does not always represent optimised decisions, on account of general equilibrium effects (Zilberman et al., 2008) and institutional uncertainties in the developing world (Wunder, 2008). Our meta-analysis of the currently available causal statistical evidence leans towards supporting these viewpoints.

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[4] More formally: $ln\frac{Pr(Higher\ rated\ livelihood\ outcome)}{1-Pr(Higher\ rated\ livelihood\ outcome)}\Big|_{payment+1} =$
$ln\frac{Pr(Higher\ rated\ livelihood\ outcome)}{1-Pr(Higher\ rated\ livelihood\ outcome)}\Big|_{payment} + 1.04$

[5] That is, we find that $\left[\widehat{Pr}(y=3|payment=0.5, voluntary\ participation=1)=0.53\right] >$
$\left[\widehat{Pr}(y=3|payment=1, voluntary\ participation=0.5)=0.41\right]$.

[6]This is calculated by using Model 1 and keeping all other variables at mean levels. That is:
$\widehat{Pr}(y=2|voluntary\ participation=1)=0.51$ and $\widehat{Pr}(yd=3|voluntary\ participation=1)=0.4$.

Table 2 Meta-regression results

Dependent variable: Livelihood impact of PES **Explanatory variables:**	**Model 1 Ordinal**	**Model 2 Ordinal, weighted**	**Model 3 Ordinal, weighted**	**Model 4 Binary**	**Model 5 Binary, weighted**	**Model 6 Binary, weighted**
Payment	1.04**	0.99**	0.94	1.16**	1.17**	0.63
	[0.43]	[0.40]	[0.69]	[0.51]	[0.54]	[0.78]
Voluntary participation	1.95***	1.98***	3.68***	3.35***	3.53***	4.92***
	[0.70]	[0.68]	[1.18]	[1.22]	[1.09]	[1.68]
Transaction costs	-4.67***	-3.40***	-6.95***	-3.93***	-2.82**	-5.51***
	[1.09]	[0.95]	[1.29]	[1.44]	[1.31]	[1.89]
Governmental programme	-0.79	-0.78	-2.01**	-0.68	-0.85	-1.14
	[0.67]	[0.61]	[0.96]	[1.16]	[1.11]	[1.25]
Alternative income sources	2.69***	1.47*	3.15***	4.33**	3.00**	2.66**
	[0.90]	[0.85]	[1.08]	[2.05]	[1.26]	[1.09]
Communal infrastructure	-3.05**	-2.29*	-4.89***	-6.26*	-5.56**	-12.5*
development	[1.35]	[1.40]	[1.63]	[3.45]	[2.28]	[6.48]
Time span	0.19	0.26**	0.48***	0.60**	0.62***	0.72***
	[0.14]	[0.13]	[0.18]	[0.25]	[0.21]	[0.20]
Communal infrastructure	0.51**	0.46*	0.88***	0.68	0.66**	2.15
development × time span	[0.23]	[0.25]	[0.33]	[0.49]	[0.33]	[1.33]
Observable confounder	-0.06	0.43	0.59	0.45	1.91	2.63*
control	[0.65]	[0.76]	[1.03]	[1.45]	[1.69]	[1.52]
Unobservable confounder	2.24***	2.51***	3.77***	2.69	2.19	3.04***
control	[0.68]	[0.90]	[0.98]	[1.95]	[1.57]	[1.01]
Baseline data	0.2	-0.18	-1.04	2.23	2.17	1.29
	[0.55]	[0.58]	[0.86]	[1.61]	[1.61]	[1.11]
Livelihood indicator	-0.74	-1.31**	-0.71	-3.00	-2.8	-2.62**
	[0.55]	[0.61]	[0.79]	[1.92]	[1.71]	[1.29]
Publication status	1.11**	0.81*	1.18**	1.08	0.97*	2.11*
	[0.43]	[0.42]	[0.56]	[0.72]	[0.55]	[1.26]
Weights		$\frac{1}{N_S}$	$\frac{1}{N_P}$		$\frac{1}{N_S}$	$\frac{1}{N_P}$
Model significance (*p*-value)	0.00	0.00	0.00	0.00	0.00	0.00
Obs.	159	159	159	159	159	159
Pseudo R^2	0.18	0.20	0.37	0.36	0.40	0.47

Note:

1. Ordinal dependent variable: y = 0 (significant negative livelihood impact), y = 1 (insignificant negative livelihood impact), y = 2 (insignificant positive livelihood impact), and y = 3 (significant positive livelihood impact). Binary dependent variable: y = 1 (significant positive livelihood impact), and y = 0 (otherwise).
2. * *p*-value < 0.10. ** *p*-value < 0.05. *** *p*-value < 0.01. Standard errors are in brackets.
3. N_S (N_P) represents the total number of data points obtained from the same study (PES programme).
4. We have omitted constants for brevity.

Turning to Hypothesis 4 regarding the implications of transaction costs, we find that the estimated coefficients for the variable 'transaction costs' are negative, sizeable and highly significant. This finding is considerably robust to different model specifications and estimation methods. These results, thus, lend support to Hypothesis 4 that PES programmes are more likely to arrive at inferior livelihood outcomes if transaction costs exceed 10% of the programme budgets. Nevertheless, transaction costs are more of a hurdle that need to be overcome than a design feature of PES. Examples of suggested remedies include bundling different ES objectives into one scheme and encouraging group applications for participation (Engel, 2016; Wunder, 2008). However, there often exist trade-offs between mitigating transaction costs and achieving cost-effectiveness, as well as environmental and distributional objectives (Börner et al, 2017). For instance, though it is argued that using auctions could deliver cost savings by allocating PES contracts to those willing to provide more desirable ES at lower prices, they may also add to the administrative complexity and costs in running the PES programme. Similarly, conditioning payments on conservation outcomes rather than actions would likely enhance the environmental efficacy of PES, but also require rigorous and costly monitoring (Hanley, Banerjee, Lennox, & Armsworth, 2012). Prioritising the participation of poor ES suppliers perhaps contributes towards poverty alleviation goals, but may also incur higher transaction costs (Pagiola et al, 2005). These trade-offs should be thoroughly considered and carefully balanced when designing PES contracts as our empirical evidence suggests that transactions costs do in fact adversely impact the livelihood implications of PES.

With respect to other attributes of PES, we find that providing alternative income sources helps foster livelihood improvements for ES suppliers. PES programmes often seek to deliver ES via avoiding forest conversion (to agriculture) or retiring agricultural lands, such as the Costa Rican PSA and the Mexican PSAH programmes. This is in essence paying ES suppliers for not utilising a particular natural resource (Ferraro & Kiss, 2002). The ensuing surplus labour could then be allocated to other income generating activities. However, it is commonly observed that populations living in developing economies heavily depend on natural resources for livelihoods, which implies their limited access to alternative livelihood sources (Barbier, 2010). A PES programme that provides or facilitates such access would thus help realise the potential of labour released by PES and thereby further benefiting ES suppliers. Results from our best available synthesis meta-analysis suggest that this reasoning is in fact supported by the available empirical evidence: PES programmes that allow for and promote alternative income sources positively impact livelihoods.

The estimates for the variable 'communal infrastructure development' are negative and considerably large in absolute value. Its interaction with 'time span' picks up a positive long term effect on the livelihood outcomes, though much smaller in magnitude compared to the main effect. This finding suggests that the net benefits of making PES payments either partly or mostly in the form of communal infrastructure investments (instead of direct cash/in-kind transfers to ES suppliers) may take a sufficiently long time to materialise.

Further, we find no clear or stable evidence for the livelihood impacts of other moderators specified in Section 2.2. The estimated coefficient of ‘governmental programme’ is consistently negative in all our models, but is insignificant in most cases. This finding to some extent casts concerns over the livelihood outcomes of governmental PES programmes in developing countries, yet the evidence remains tentative. The estimates for ‘time span’ are mostly positive and significant, yet we observe swings in their signs and statistical significance, especially in Models C1–C3 in Appendix C. Hence, we should be cautious in generalising the positive results in Table 2. Moreover, we explored adding interaction terms between ‘time span’ and other PES features to the meta-regression models. Only the interaction with ‘communal infrastructure development’ was found to yield stable and significant estimates. Hence, based on the current body of robust empirical studies, the jury is still out on the impacts of these two important aspects of PES. Further PES evaluation studies would be needed before their impacts on livelihoods assertions are reassessed.

Lastly, we turn to the meta-regression results regarding the characteristics of the empirical strategies and publication status of the sampled evaluation studies. The effects of ‘unobservable confounder control’ are noteworthy. This dummy variable equals to one if the livelihood impact is gauged by those empirical methods more capable of controlling for some determinants of the livelihood outcome (other than the PES treatment) which are unidentified or difficult to measure. These methods include panel models, instrumental variables and randomised controlled trials (Angrist & Pischke, 2009). This finding suggests that in order to provide more convincing evidence for the causal relationship between a PES programme and the outcomes of interest, it should be helpful to cross-validate the results using these empirical strategies that control for unobservable confounders. Further, we find suggestive evidence that impact evaluation studies are less optimistic about the livelihood outcomes of PES when using non-income livelihood indicators. The estimates for ‘observable confounder control’ and ‘baseline data’ yield a less clear picture. Lastly, the coefficient of ‘publication status’ is consistently positive and significant in nearly all models, pointing towards a preference for publishing positive results of PES over null or negative ones. This finding further justifies the necessity of including both published and unpublished evaluation studies in our meta-analysis. It also highlights the possible bias on behalf of journals of not publishing studies with null or negative results.

6 Conclusion

This study conducts perhaps a first meta-analysis on the direction, magnitude and influencing factors of the livelihood impacts of PES on ES suppliers in developing countries. The analysis consists of 161 data points from 27 studies. These empirical estimates of livelihood impacts (i.e. effect sizes) were derived from the existing body of primary studies that explicitly adopts causal statistical methods in an attempt to assess the causal link between PES and livelihoods (as opposed to simple correlational studies or qualitative methods such as structured interviews). This approach for delineating eligible studies allowed us to derive a synthesis of the more methodologically cohesive empirical evidence. The hypotheses that were tested were derived from often made assertions and stylised facts found in the relevant literature on the likely livelihood impacts of PES and the determinants of these impacts. The set-up of our meta-analysis allows us to draw more conservative but more *generalisable overview* lessons on these assertions compared to the conclusions derived on the basis of isolated case studies or reviews that use evidence generated through a much wider spectrum of methods.

The estimates of the typical effect size indicate that PES programmes do have a positive livelihood impact on ES suppliers, albeit a modest one at the 10% significance level. The meta-regression models provide supporting evidence for the hypotheses that PES programmes are likely to have more favourable livelihood impacts if they provide higher payments (either cash and in-kind), entail higher degrees of voluntary participation, incur lower transaction costs and create alternative income sources. Furthermore, we find that making PES payments partly/mostly in the form of communal infrastructure investments would only benefit ES suppliers in the long term. Our best evidence based meta-analysis does not provide robust support for other PES attributes frequently alleged to be influential for the livelihood implications for ES suppliers. These attributes include whether a PES programme is financed and executed by governmental bodies, and how much time has passed after its introduction. Their effects may be present in isolated case studies but the point being made in this paper is that we find that the available statistical causal evidence does not allow (at least for now) for these assertions to be generalised. Last but not least, our results have also identified the necessity of controlling for unobservable confounders to cross-validate the estimated treatment effects of PES in impact evaluation studies.

On the basis of these more evidence based generalisable findings, we can infer useful implications for the design, implementation and evaluation of PES. PES are hatched as 'win-settle' programmes which have as a primary aim to provide additional ES whilst ensuring that ES suppliers are not made worse off (Wunder, 2013). In spite of that, it is instrumental for the delivery of ES to ensure that a PES scheme is beneficial to the livelihoods of ES suppliers. The underlying assumption is that additional ES supply nearly always conflicts with certain income generating activities and thus cannot be guaranteed if the losses are not fully compensated (Engel et al., 2008; Engel 2016; Pearce, 2004). Following this logic, PES programmes analysed in this study have (on average) met this prerequisite for additional ES provision. This makes us conservatively optimistic about the average environmental efficacy of these PES programmes based on the current evidence.[7] That said, a nontrivial proportion of these PES programmes were found to have caused (significant or insignificant) net losses to ES suppliers. Such results provide a cautionary warning that further improvements to livelihoods must be incorporated in this subset of PES programmes to secure ES provision. Our meta-analysis suggests that the most straightforward means of achieving this is to

[7] Of course, this interpretation is largely speculative and relies heavily on the more narrow economic understanding of PES programmes. Net gains for ES suppliers do not necessarily secure *additional* ES provision if: (1) ES suppliers are paid for ES that could have been delivered without compensation (no opportunity costs); (2) activities afflicting ES supply are displaced to locations that are not bound by PES obligations (leakage effects); and/or (3) PES payments weaken ('crowd out') non-economic incentives for ES provision such as civic virtues, etc. (Muradian et al., 2013; Pattanayak et al., 2010; Vatn, 2010). Ideally future research would undertake a meta-analysis of the causal statistical evidence of the environmental outcomes of PES to substantiate these conjectures. To date, this is not feasible given the limited number of relevant causal statistical studies.

increase the payment rate. Yet, more importantly our analysis highlights other PES design attributes that are important determinants of livelihood improvements, such as the degree of voluntary participation, the level of transaction costs and the availability of alternative income sources. Simply handing out cash alone may not suffice to ensure prolonged significant benefits for ES suppliers.

Furthermore, the limited size and significance of the positive livelihood benefits identified in the meta-analysis could enhance the risk that these modest gains could be reversed after PES payments end, which would put the associated ES that are supplied at stake. Infinitely extending payment durations (and contract periods) would be prohibitively difficult, if not impossible (Engel, 2016). Nevertheless, it is possible to induce a continuing supply of ES beyond the finite time horizon of PES payments. This can be achieved by designing PES schemes in a way that they directly address certain institutional and market failures that led to the under provision of the ecosystem services in the first place (Grosjean & Kontoleon, 2009). For example, property right ambiguities may compel land holders to intensify and/or expand cultivation as a means to secure *de facto* land rights. This practice could adversely affect the levels of ecosystem services (by exacerbating loss of topsoil and siltation of source waters) and land holders' livelihoods (by obstructing more profitable alternative income generating activities such as pursuing off-farm employments and growing fruit trees). There is a similar example concerning liquidity constraints, namely that subsistence farmers cannot afford upfront investments in livelihood enhancing activities due to lack of cash flows, and are thus locked into sub-optimal production activities that generate less income at a higher cost of natural resources. Perhaps PES programmes should specifically include built in components intended to address these labour market institutional impediments. This would enhance the long-term viability or sustainability of ES provision.

It is worth noting that our findings are confined to the narrower economic or financial dimension of livelihoods. A much wider array of assets and activities jointly contribute to the living standards and well-being of ES suppliers (Chambers & Conway, 1992; Hejnowicz et al., 2014). As can be seen in Table 1, we find that PES programmes on average have an insignificant positive effect on the financial capital of ES suppliers. But this result may not proffer unambiguous implications for other livelihood components, such as natural capital, social capital and institutional capital. Taking natural capital as the first example, we have mentioned that the centrepiece of PES design presumes that net financial benefits would incentivise ES provision, which often relates to the enhancement of natural capital. Yet, we have discussed that such improvements in natural capital may be undermined by leakage effects (Pattanayak, Wunder, & Ferraro, 2010). Considering social capital, whether PES payments weaken ('crowd out') or strengthen ('crowd in') conservation related civic virtues and behaviour is largely unpredictable and context dependent (Wunder, 2013). Moreover, we have seen inconsistent (and even opposite) effects of different PES cases on institutional capital, such as the development of property rights (Arriagada et al., 2015; Liu, Gong, & Kontoleon, 2018; Phelps et al., 2010; Wunder, 2008). More attention needs to be drawn to empirically assessing the impacts of PES on other facets of livelihoods, in particular, human, social and institutional capital (Hejnowicz et al., 2014).

Our results and recommendations do not of course provide the final word on the performance of PES on livelihoods. *Ex post* empirical evidence is inherently drawn from past experiences and cannot fully account for PES cases that are currently ongoing. Instead, we have argued that our approach does provide for a more objective and reliable account of which of the assertions made in the literature on the livelihood impacts of PES can and which cannot be supported by the current body of causal statistical empirical studies. Meta-analysis is an ever evolving exercise that needs to be repeated as more studies become available. Nevertheless, periodically taking stock in a systematic way of what current best practice evidence can support aids the advancement of science and policy making. The plea made by many authors for the urgent need for undertaking more methodologically sound empirical studies that evaluate PES (Ferraro and Pattanayak 2006; Miteva et al., 2012) is motivated primarily by the acknowledged necessity to increase the body of empirical evidence in order to undertake further meta-analyses in the future.

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Appendix A   Searching Strategies and Inclusion Criteria

A.1   Searching strategies

The following sources and searching strategies were used to search for primary evaluation studies on the livelihood impacts of PES to construct the sample of the meta-analysis.

Firstly, several academic digital databases were searched, including the Web of Science (formerly known as the ISI Web of Knowledge), Science Direct and IDEAS. Google search engine was also used and the first 200 returns were screened, in order to cover unpublished and grey literature including working papers, conference papers and degree dissertations. Three categories of key words were used to search those sources. The first category identified 'payments', including: 'pay*', 'compensat*', 'incentiv*', 'reward*', 'award*', and 'subsid*'. The second category identified 'environment', including: 'ecol*', 'ecos*', 'environment*' and 'conservation*'. The third category identified 'impacts', including: 'impact*', 'effect*', 'livelihood*', 'povert*', 'socioeconomic*' 'income*' 'wel*'. We searched for all possible combinations of the three categories of keywords, and used wildcards (*) and fuzzy searching wherever possible.

Furthermore, the online libraries of some highly-relevant non-governmental organisations (NGOs) were also searched, including the International Initiative for Impact Evaluation (3ie), International Institute for Environment and Development (IIED), Abdul Latif Jameel Poverty Action Lab (J-PAL), Environment for Development (EfD), Economy and Environment Program for Southeast Asia (EEPSEA), and Collaboration of Environmental Evidence. The titles and abstracts of all the research reports from those sources were browsed one by one, instead of searching by keywords.

Moreover, the online collections of some highly-relevant academic journals were used, including the Journal of the Association of Environmental and Resource Economists, the Journal of Environmental Economics and Management, the American Journal of Agricultural Economics, Environmental and Resource Economics, Land Economics, World Development, Ecological Economics, Environment and Development Economics, Environmental Conservation, Ecosystem Services and the Journal of Agricultural Economics. The titles and abstracts of the papers were browsed one by one. Only the issues from 2000 to present were searched because causal empirical literature on PES was rare before that date   After accumulating a certain number of studies for potential inclusion in the meta-analysis, backward and forward searches were conducted to collect more studies on the topic. 'Backward searching' refers to the process of searching for relevant studies cited by the literature that has already been collected. In contrast, 'forward searching' aims to find studies that cite the literature already collected. The forward searches were conducted using the Web of Science database.

We sent out 19 emails to the corresponding authors of those sampled studies and other researchers in the most relevant research area. We received responses from 8 of these researchers, which represents a response rate of roughly 42%. Those emails were used to request for, 1) additional studies on the topic, and 2) supplementary information about those studies already sampled.

A.2 Inclusion criteria

We included only those studies that, 1) conducted *ex post* evaluations of the livelihood impacts of PES on *ES suppliers* at the *household* level, 2) employed causal statistical methods (such as randomised controlled trials, regressions, matching, difference-in-difference and fixed effects, instrumental variables and regression discontinuity, etc.), and 3) measured the impact on at least one indicator which reflects the overall livelihood status such as total income, total consumption expenditure and/or gross value of household assets. Based on these criteria, studies such as those of Antle (2008), Cole (2010) and their co-authors were not included because their evaluation methods were beyond the scope of this study; studies such as those of Robalino (2014), Sims (2017) and their co-authors were not included because the impact was not measured at the household level; studies such as those of Bravo-Ureta (2011), Kwayu (2017) and their co-authors were not included because their livelihood indicators were unable to reflect the overall livelihood status of ES suppliers. This left us with 27 primary evaluation studies, as shown in Table A1.

Table A1 Sampled studies

Literature	PES programme	Location	Full sample size (households)[a]	Livelihood indicator	Evaluation method	Number of estimates of the livelihood impact	Most frequently reported estimate of the livelihood impact (number)
(Alix-Garcia et al. 2012)	PSAH	Mexico	1242	Asset	DID, Matching	12	Insignificantly positive (9)
(Alix-Garcia et al. 2015)	PSAH	Mexico	1210	Asset	DID, Matching	6	Insignificantly positive (6)
(Arriagada et al. 2015)	PSA	Costa Rica	202	Asset	DID, Matching	6	Insignificantly negative (6)
(Clements & Milner-Gulland, 2015)	BNPP	Cambodia	247	Asset	DID, Matching	1	Insignificantly negative (1)
(Diswandi, 2017)	Lombok PES	Indonesia	200	Asset	OLS/IV	1	Insignificantly positive (1)
(Groom, 2012)	SLCP	China	286	Income	DID, Matching	8	Significantly positive (4)
(Groom & Palmer, 2012)	SLCP	China	284	Income	DID, Matching	7	Significantly positive (5)
(Hegde & Bull, 2011)	N'hambita PES	Mozambique	290	Income	Matching	12	Insignificantly positive (8)
(Jack & Santos,2017)	RCT	Malawi	433	Asset	RCT, DID	2	Insignificantly positive (2)
(Jayachandran et al., 2016)	RCT	Uganda	1099	Income	RCT, DID	3	Significantly positive (2)
(Jindal et al. , 2012)	N'hambita PES	Mozambique	190	Income, asset	DID	4	Significantly positive (3)
(Kiyingi et al., 2016)	TGB	Uganda	638	Consumption	Matching, Switching regression	4	Insignificantly positive (3)
(Li et al., 2011)	SLCP	China	1074	Income	OLS/IV	11	Significantly positive (9)
(Liang et al. , 2012)	SLCP	China	1073	Income	OLS/IV	4	Insignificantly negative (2)
(Lin & Yao, 2014)	SLCP	China	269	Income	SEM	4	Insignificantly positive (4)

Literature	PES programme	Location	Full sample size (households)[a]	Livelihood indicator	Evaluation method	Number of estimates of the livelihood impact	Most frequently reported estimate of the livelihood impact (number)
(Liu et al., 2010)	SLCP	China	1968	Income	FE panel	6	Significantly positive (6)
(Liu & Henningsen, 2016)	SLCP	China	1458	Consumption	HT panel	10	Significantly positive (4)
(Lokina & John, 2016)	EPWS	Tanzania	200	Asset	Matching	1	Significantly positive (1)
(Ly, 2013)	PFES	Vietnam	749	Income	DID, Matching	8	Insignificantly positive (6)
(Martin et al., 2014)	RCT	Rwanda	357	Consumption	RCT, DID	6	Insignificantly positive (4)
(Morse, 2007)	PSA	Costa Rica	208	Income	Matching	2	Insignificantly positive (1); significantly positive (1)
(Phan et al., 2015)	PFES	Vietnam	264	Income	DID	3	Significantly positive (3)
(Swartz, 2015)	BFP	Brazil	214	Income, asset	Matching, OLS	24	Insignificantly negative (11); insignificantly positive (11)
(The & Ngoc, 2006)	RCT	Vietnam	136	Income	RCT	1	Insignificantly positive (1)
(Uchida et al., 2007)	SLCP	China	339	Income	DID, Matching	7	Insignificantly positive (5)
(Xu et al., 2010)	SLCP	China	345	Income	DID	6	Insignificantly positive (5)
(Zheng et al., 2013)	PLDL	China	723	Income	DID, Matching	2	Insignificantly positive (1); significantly positive (1)

Notes:
The full sample size refers to the maximum number of households that were surveyed. Some estimates may be derived from different sub-samples.

Abbreviations:

BFP: Bolsa Floresta Programme (Forest Conservation Grant Fund).
BNPP: Bird Nest Protection Programme.
DID: Difference-in-Difference.
EPWS: Equitable Payments for Watershed Services.
FE: Fixed Effects.
HT: Hausman-Taylor.
OLS: Ordinary Least Squares.
PFES: Payments for Forest Environmental Services.
PLDL: Paddy Land-to-Dry Land.
PSA: Pago por Servicios Ambientales (Payments for Environmental Services).
PSAH: Pago de Servicios Ambientales Hidrológicos (Payments for Hydrological Environmental Services).
RCT: Randomized Controlled Trial.
SEM: Simultaneous Equation Model.
IV: Instrumental Variables
SLCP: Sloping Land Conversion Programme.
TGB: Trees for Global Benefits.

Appendix B Coding Methods and Descriptive Statistics

The dependent and explanatory variables involved in the meta-regressions were coded into metrics according to their definitions as illustrated in Table B1. The vast majority of the required information was extracted from the 27 impact evaluation studies. Upon the occurrence of missing values, we would firstly contact the authors of these papers for such information. In case the authors were unreachable, we would impute the missing information according to alternative sources. The imputed information and sources are summarised by Table B2.

Table B1 Descriptive statistics

Unit of analysis:	Estimate	Study	PES
Variable	**Mean [SD]**	**Mean [SD]**	**Mean [SD]**
Livelihood impact (ordinal) = 0 if significant negative, = 1 if insignificant negative, = 2 if insignificant positive, and = 3 if significant positive.	2.07 [0.76]	2.13 [0.57]	2.07 [0.52]
Payment (continuous) The relative difference between PES payments (cash and in-kind) and profits of alternative land-use patterns (or production practices).	0.11 [0.63]	0.15 [0.68]	0.06 [0.60]
Voluntary participation (binary) = 1 if no involuntary participation explicitly documented (= 0 otherwise).	0.60 [0.49]	0.63 [0.49]	0.87 [0.35]
Transaction costs (binary) = 1 if over 10% of the total budget is explicitly spent on administrative affairs such as coordinating ES users and suppliers, arranging contracts, making payments and monitoring (= 0 otherwise).	0.38 [0.49]	0.37 [0.49]	0.53 [0.52]
Governmental programme (binary) = 1 if a PES programme is funded and implemented by the government (= 0 otherwise).	0.63 [0.48]	0.59 [0.50]	0.33 [0.49]
Alternative income sources (binary) = 1 if alternative income sources are made available to more than 50% of ES suppliers (= 0 otherwise).	0.43 [0.50]	0.39 [0.49]	0.49 [0.50]
Communal infrastructure development (binary) = 1 if over 50% of the payments are invested in communal infrastructure development (= 0 otherwise).	0.47 [0.50]	0.39 [0.49]	0.48 [0.51]
Time span (continuous) Years between the commencement and the evaluation of the programme.	5.24 [2.25]	4.92 [2.29]	4.31 [1.94]
Observable confounder control (binary) = 1 if the evaluation study controls for observable confounders (= 0 otherwise).	0.91 [0.29]	0.88 [0.28]	0.85 [0.28]
Unobservable confounder control (binary) = 1 if the evaluation study controls for unobservable confounders (= 0 otherwise).	0.79 [0.41]	0.80 [0.39]	0.82 [0.33]
Baseline data (binary) = 1if the evaluation study uses baseline data (= 0 otherwise).	0.65 [0.48]	0.67 [0.45]	0.67 [0.37]
Livelihood indicator (binary) = 1 if the impact evaluation study uses non-income livelihood indicators (= 0 otherwise).	0.40 [0.49]	0.41 [0.48]	0.57 [0.46]
Publication status (binary) = 1 if the evaluation study has been published as a journal article or a monograph (= 0 otherwise).	0.62 [0.49]	0.70 [0.47]	0.66 [0.45]
Full sample size	161	27	15

Table B2 Imputed information and sources

Literature	Imputed information and sources
(Alix-Garcia et al., 2012)	Profits of alternative land-use (corn and livestock production): Muñoz-Piña (2008).
(Alix-Garcia, et al. 2015)	Profits of alternative land-use (corn and livestock production): Muñoz-Piña (2008).
(Arriagada, et al. 2015)	Programme payments: Pagiola (2008); profits of alternative land-use (rent as pasture) and transaction costs: Porras et al. (2013).
(Clements & Milner-Gulland, 2015)	Programme payments, daily wage and transaction costs: Clements et al. (2013).
(Diswandi, 2017)	Profits of alternative land-use (none): communications with the author.
(Groom, 2012)	Profits of alternative land-use (grain production): China Rural Statistical Yearbook (National Bureau of Statistics, 2003-2011).
(Groom & Palmer, 2012)	Profits of alternative land-use (grain production): China Rural Statistical Yearbook (National Bureau of Statistics, 2003-2011).
(Hegde & Bull, 2011)	Programme payments and transaction costs: Jindal et al. (2012).
(Jack & Santos,2017)	Programme payments and transaction costs: Jack (2013); profits of alternative land-use (maize and soya production): Jack (2011).
(Jayachandran et al., 2016)	Profits of alternative land-use (cutting trees and cultivation) and transaction costs: Jayachandran et al. (2017).
(Jindal, Kerr, & Carter, 2012)	Profits of alternative land-use (crop production): Hegde and Bull (2011).
(Kiyingi et al., 2016)	Programme payments and alternative land-use (coffee production): Fisher (2011).
(Li, Feldman, Li, & Daily, 2011)	Programme payments: Xu et al. (2010); profits of alternative land-use (grain production): China Rural Statistical Yearbook (National Bureau of Statistics, 2003-2011).
(Liang, Li, Feldman, & Daily, 2012)	Programme payments: Xu et al. (2010); profits of alternative land-use (grain production): China Rural Statistical Yearbook (National Bureau of Statistics, 2003-2011).
(Lin & Yao, 2014)	Profits of alternative land-use (grain production): China Rural Statistical Yearbook (National Bureau of Statistics, 2003-2011).
(Liu, Lu, & Yin, 2010)	Profits of alternative land-use (grain production): China Rural Statistical Yearbook (National Bureau of Statistics, 2003-2011).
(Morse, 2007)	Programme payments: (Pagiola, 2008); profits of alternative land-use (rent as pasture) and transaction costs: Porras et al. (2013).
(Swartz, 2015)	Programme payments and profits of alternative land-use (cultivation): Lucas (2013).
(The & Ngoc, 2006)	Livelihood impact: communications with authors.

Note: No information was imputed for the other studies in our sample.

Appendix C Robustness Checks

Table C1 Robustness checks for the livelihood impact of PES programmes

Livelihood impact	Unit of analysis: Estimate	Study	PES programme
Panel 1: Mode			
Number (percentage) of significant negative estimates: $y = 0$	0 (0%)	0 (0%)	0 (0%)
Number (percentage) of insignificant negative estimates: $y = 1$	24 (25%)	3 (15%)	3 (20%)
Number (percentage) of insignificant positive estimates: y = 2	***53 (55%)***	***11 (55%)***	***9 (60%)***
Number (percentage) of significant positive estimates: $y = 3$	20 (20%)	6 (30%)	3 (20%)
Total number (sample size)	97 (100%)	20 (100%)	15 (100%)
Panel 2: Mean			
Mean [standard deviation]	1.96 [0.68]	2.14 [0.60]	2.09 [0.58]
Sample size	97	18	14
p-value from *t*-test: mean = 0 (significant negative impact)	0.00	0.00	0.00
p-value from *t*-test: mean = 1 (insignificant negative impact)	0.00	0.00	0.00
p-value from t-test: mean = 2 (insignificant positive impact)	***0.55***	***0.33***	***0.58***
p-value from *t*-test: mean = 3 (significant positive impact)	0.00	0.00	0.00

Note: These robustness checks are based on a subset of our data that drops observations on involuntary ES suppliers.

Table C2 Robustness checks for the meta-regression results

Dependent variable: Livelihood impact of PES **Explanatory variables:**	**Model C1 Binary**	**Model C2 Binary, weighted**	**Model C3 Binary, weighted**	**Model C4 Ordinal**	**Model C5 Ordinal, weighted**	**Model C6 Ordinal, weighted**
Payment	0.28	0.52	1.46*	1.86	1.24	2.04*
	[0.62]	[0.57]	[0.80]	[1.23]	[0.97]	[1.25]
Voluntary participation	2.26**	3.04***	6.11***			
	[1.14]	[1.10]	[1.62]			
Transaction costs	-5.79***	-5.39***	-8.16***	-11.68***	-10.67***	-13.83***
	[1.24]	[1.22]	[1.59]	[3.73]	[2.27]	[4.08]
Governmental programme	-1.19	-1.36	-0.99	-1.58	-2.24	-2.50*
	[1.06]	[1.00]	[1.09]	[1.22]	[1.44]	[1.41]
Alternative income sources	2.36***	0.91	4.30**	7.89*	6.16***	8.56**
	[0.86]	[0.75]	[1.78]	[4.07]	[2.25]	[4.15]
Communal infrastructure	-3.99	-0.65	-2.38	-6.34**	-5.70*	-7.60***
development	[2.45]	[3.05]	[2.95]	[2.88]	[3.02]	[2.50]
Time span	-0.06	0.19	0.25	0.87***	1.09***	1.12***
	[0.16]	[0.16]	[0.19]	[0.32]	[0.30]	[0.38]
Communal infrastructure	0.97**	0.64	0.75	0.71	0.72	0.94
development × time span	[0.46]	[0.61]	[0.84]	[0.65]	[0.67]	[0.61]
Observable confounder	1.21	1.49	2.73**	-0.69	-0.81	-0.60
control	[1.12]	[1.10]	[1.34]	[1.25]	[1.25]	[1.33]
Unobservable confounder	5.83***	5.85***	6.10**	4.22***	4.78**	4.97***
control	[2.10]	[2.04]	[2.92]	[1.43]	[1.92]	[1.58]
Baseline data	-2.87	-3.08	-2.16	-0.30	-1.28	-0.92
	[2.16]	[2.05]	[2.31]	[1.18]	[1.14]	[1.44]
Livelihood indicator	-0.73	-2.50**	-2.52	-0.29	-1.24	-0.39
	[0.94]	[1.14]	[1.55]	[0.98]	[0.97]	[1.12]
Publication status	1.89**	2.14**	3.98**	1.81**	1.02	2.13*
	[0.78]	[0.95]	[1.73]	[0.91]	[0.68]	[1.21]
Weights		$\frac{1}{N_S}$	$\frac{1}{N_P}$		$\frac{1}{N_S}$	$\frac{1}{N_P}$
Model significance (*p*-value)	0.00	0.00	0.00	0.00	0.00	0.00
Obs.	159	159	159	95	95	95
Pseudo R^2	0.31	0.39	0.57	0.40	0.49	0.52

Note:

1. Binary dependent variable: y = 0 (negative livelihood impact), and y = 1 (positive livelihood impact). Ordinal dependent variable: y = 0 (significant negative livelihood impact), y = 1 (insignificant negative livelihood impact), y = 2 (insignificant positive livelihood impact), and y = 3 (significant positive livelihood impact).
2. Models C4–C6 are estimated using a subset of our data that drops observations on involuntary ES suppliers. The regressor 'voluntary participation' is thus dropped from these models because it has no variation.
3. * p-value < 0.10. ** p-value < 0.05. *** p-value < 0.01. Standard errors are in brackets.
4. N_S (N_P) represents the total number of estimates obtained from the same study (PES programme).
5. We have omitted constants for brevity.